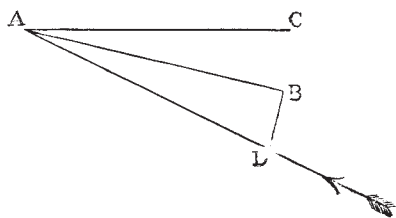


posture of the bird very well for a few seconds, till he became aware of my presence and dashed away. I was much struck by those instances in which the obstacle that caused the upward slant of the wind was only a sea-wall or a railway embankment, and especially by the critical case (No. 12) where the bird was evidently baffled because the wind lay along the embankment, not against it, and therefore gave no upward current.

My list includes four cases (3, 4, 5, 6) of rooks and gulls "up-borne" on outspread wings, under conditions similar to those present in hovering—cases that could not be explained by any theory of *vis viva*, but clearly involved an external mechanical force, which could only be that of the wind, sustaining and uplifting the birds. The close relation between the "up-borne" and the "hovering" action was evident in case (9) where the gulls, &c., were up-borne and sailing, while the hawk was poised and motionless.

These observations, as far as they go, appear to indicate plainly the law which governs the phenomenon in question. I think they strongly confirm the theory already advanced, that the bird in hovering is upheld by a slant upward current of air. A strong wind pressing against a slope of ground is necessarily thrown into a slant upward current, "as slopes a wild brook o'er a hidden stone." There may be a downward eddy if the slope is precipitous, as one may often feel at the foot of a high wall, but the main stream of the air for some considerable height above the slope is forced to take an upward slant, with increased velocity, in order to surmount the obstacle in its path.

Given such a slant upward current, it is easy to see



that a bird, with the exquisite muscular sense that every act of flight demands and denotes, might so adapt the balance of its body and the slope of its wing-surface to the wind as to remain motionless in relation to the earth. The slope of the wing-surface should divide the angle between the horizontal and the direction of the slant wind-current in such proportion that, if the air were at rest, the bird, under the action of gravity, would float forwards, downwards, on outspread wings, with exactly the same velocity as that of the wind (in which it remains motionless) and in exactly the opposite direction. The mechanical conditions are identical in the two cases, whether we consider the air at rest and the bird floating through it, or the bird at rest and the wind rushing under it. In either case the bird has reached, and maintains, its maximum velocity, due to gravity, compatible with the resistance of the air, which resistance is the same in both cases.

I have heard it objected that the mechanical conditions are not the same in these two cases, because in the one case the bird has momentum, in the other not. Need it be said that momentum is a purely relative possession, just as velocity is? In each case the bird has the same velocity, and therefore the same momentum, relative to the air. The mechanics of the situation, as between bird and air, are not affected by the possession or loss of velocity (and with it momentum) relative to the earth.

Perhaps the feasibility of the thing may be best shown by a simple diagram. Let AB represent the slope of the bird's wing (viewed from the right side), dividing the angle between the horizontal, AC, and the direction of

the wind, DA. Draw BD at right angles to AB, and take AD to represent the force of the wind. Then DB and BA will represent the force of the wind resolved perpendicular and parallel to the slope of the wing AB. The resolved part, BA, meeting only the resistance of the bird's head and shoulders and front edge of the wings, tends not strongly to push the bird in the direction, BA, that is, backwards and a little upwards. But the resolved part, DB, which meets the full area of the outspread wings and tail, tends powerfully to push the bird in the direction DB, that is, upwards and a little forwards. Then all that is required to keep the bird at rest is that the effect of the forward force exerted by DB should balance the effect of the backward force exerted by BA (both being resolved vertically and horizontally), and that the great upward force exerted by DB, together with the small upward force exerted by BA should exactly neutralise the downward force of gravity.

The only difficulty in the way of the slant-upward-current theory lies in the statement of the Duke of Argyll (NATURE, vol. x. p. 262) that "a hundred times" he has seen birds hovering "when by no possibility could any upward deflection of the wind have arisen from the configuration of the ground." My own observations testify so consistently in favour of slant upward currents that I feel justified in asking for more precise information concerning the instances alluded to by the Duke of Argyll, before relinquishing the theory which I hold. Wherever I have seen a hawk trying to remain in one position over a plain or slightly undulating ground, the feat has only been accomplished by continued vibration of the wings.

The problem of the "soaring" of birds introduces other conditions, which require separate consideration, though I believe it will be found that the two phenomena of "soaring" and "hovering" depend upon essentially similar causes.

(By the bye, does not the provincial name of one of the hawks, the "Windhover" record the constantly observed dependence of the act of hovering on the wind?)

HUBERT AIRY

THE LATE EDWARD B. TAWNEY

BY the death of this young naturalist English geology has lost one of its most enthusiastic and cultivated students. Hardly beyond the threshold of his career, he had already gained for himself a notable place among the geologists of this country, and his friends augured for him a future of distinction and usefulness. But in the fulness of his promise and in the midst of his work he has been struck down so suddenly that few of his friends knew he had been ailing until they were shocked and saddened by the news of his death.

Born in 1841, he was the third child of the Rev. Richard Tawney, Vicar of Willoughby, Warwickshire, who had gained a distinguished place at Rugby, and had been a Fellow of Magdalen College. On the death of his father, young Tawney was placed under the care of his guardian, Dr. Bernard of Clifton, and received his early education there. During these years he seems to have acquired a bent towards natural science mainly through the influence of Dr. Bernard and Dr. Fox of Bristol. He was eventually enabled to gratify this inclination by attending the courses of instruction at the Royal School of Mines, Jermyn Street, from 1860-63, where he greatly distinguished himself. He gained a Royal Scholarship, Duke of Cornwall's Scholarship, the De la Beche Medal for Mining, and the Edward Forbes Medal for Natural Science, and took Associate's diplomas in the Mining and Geological divisions.

With the training in scientific methods thus obtained, he soon betook himself to original research, gaining experience by excursions at home and by travel abroad. In 1872 he accepted the offer of Assistant Curator of the

Bristol Museum. With characteristic energy he at once set to work, re-tableting, re-arranging, and naming the geological collection, taking care to have gaps in the series filled up, and making the museum really serviceable for purposes of instruction. Six years later, in the early part of 1878, he received the appointment of Assistant Curator of the Woodwardian Museum, Cambridge. He soon made his mark there, as was acknowledged in the following year by the bestowal upon him of the honorary M.A. degree. His indefatigable industry and wide range of acquirements so peculiarly fitted him for this position, that his death must for some time to come be an almost irreparable loss to the University.

Looking over his published papers one cannot but be struck with his versatility. At one time we find him discussing the Rhoetic beds of South Wales, at another dealing with that vexed question of Alpine geology—the position of *Terebratula diphya*. From Devonian fossils he passes to the description of new species of Oolitic gasteropods, or to the Cretaceous Aporrhaidæ, or to Palæozoic star-fishes. He could enter minutely into the stratigraphy of the Isle of Wight Tertiary strata, and with not less energy and clearness of insight described the microscopic structure of the crystalline rocks of Wales. Well versed in the Continental languages, he kept himself abreast of the foreign progress of his favourite science. Nor were his tastes wholly scientific. He delighted in Piers Ploughman and the Niebelungenlied. What he might have done who may guess? That with his feebleness of constitution he should have been able to accomplish so much, shows how ardent was his love of nature and how indomitable his spirit of inquiry. His devotion to truth and abhorrence of everything savouring of insincerity or sham led him to speak out freely and uncompromisingly. But no one could mistake the honesty of his purpose. A. G.

REMARKS ON AND OBSERVATIONS OF THE METEORIC AURAL PHENOMENON OF NOVEMBER 17, 1882

THE interesting meteoric phenomenon seen in England during the aurora of November 17 last, has induced me to endeavour to find the true path of that object. Though I have spent much time in applying the method given by Prof. E. Heis in his "Periodischen Sternschnuppen," I have got no farther than the point to which Mr. H. D. Taylor has brought us, the observations being in no way capable of combining. In fact, when seeking the lines of intersection, formed by the different planes of the great circles, wherein the apparent path was seen, with the mean horizon (say the plane of a common map), these lines have but little tendency to converge to the same point. Therefore the method of Mr. Taylor seems to me the most convenient. When the object has followed a straight line, all the places where it was seen passing just before the moon, must lie in a plane containing the true path and the moon. This plane must cut the plane of the map in a straight line. Now the four places where observers saw the meteor before the moon's disc are:—Woodbridge, near Ipswich, Lincoln's Inn Fields (London), Windsor, and Ramsbury, near Hungerford, fulfilling, by no means, the above-mentioned condition. Nevertheless the most probable direction of this line seems to be that accepted by Mr. Taylor, N.E. by E.-S.W. by W. (astronomical), because this is the general direction of the lines of section, given by the great circles, mentioned above. Here it is to be remarked that when the meteor was seen from S.E. to S.W. (as in the case at York), but at some height (here 10°) above the horizon, the intersections of the apparent path with the horizon may lie near E. and W. (here, according to the observation of the meteor passing 6° below the moon, at 12° south of E.). We give here

bearings as seen from the different places, taken directly from the communications, or deduced indirectly from them:—

Street (3° south of Leeds)	S.E.-S.W.
Clifton (Bristol)	E. 18° N.-W. 18° S.
Greenwich	E.N.E.- (?)
Guildown (p. 149)	E.-W. (nearly).
Bedford	S.E.-S.W.
Clevedon (p. 100)	N. 70° E. (?) - S. 70° W.
Cambridge	E.-S.S.W.
York (H. D. Taylor)	E. 12° S.-W. 12° N.
Woodbridge	E. 10° N.-W. 10° S.
Windsor	E.-W.
Coopers Hill	?-S.W.
Ramsbury	?
Lincoln's Inn Fields	E.-W.

Now we can add to these English observations¹ two others made in the Netherlands.

1. Prof T. A. C. Oudemans gives in the *Utrecht Newspaper* (No. 318) the following (translated) description:—"At 6h. 23m. (6h. $2\frac{1}{2}$ m. Greenw. T.) a feather-like appearance, resembling in the beginning a brilliant comet, formed suddenly in the eastern part of the heavens, the end being just before Aldebaran. Within two minutes this feather had prolonged itself above Saturn, through the Pegasus quadrature, and south of the three Eagle-stars, the east or following end shortening, while the other or preceding end advanced. . . . When this arch had obtained the length of 90° (which lasted but a few seconds) a separation was made in the middle of its length, where the arch had a breadth of about 3° . This separation had a length of about 10° and a breadth of $\frac{1}{2}^\circ$, and was pointed at the ends. At 6h. 25m. this arch disappeared wholly in the west." Prof. Oudemans says further that the great circle of the apparent path intersected the equator at 110° and 290° of right ascension. This gives me, combined with the position of Aldebaran, a direction in the horizon of E. 20° N.-W. 20° S.

2. Mr. P. Zeeman observed the same phenomenon at at Zonnemaire, near Zierikzee ($51^\circ 42'$ lat. and $57'$ W. Amsterdam). He wrote me the following on November 19 and 24:—"About 6h. 20m. (I saw) a magnificent, splendid white arch, beginning a little north of east, and prolonging itself to south-west, but in the meantime shortening at the east end and disappearing in a very short time." Mr. Zeeman declares in his second letter that this arch went through Aldebaran, and through a Pegasus. This gives me a horizontal bearing of E. 20° N.-W. 20° S., as the observations of Prof. Oudemans gives also.

Thus we find these two Dutch observations (unhappily the sky in Groningen had just, at 6h. 1m. Greenw. T., got cloudy, the aurora being very splendid before) supplement and confirm the greater part of the English observations. Only the phenomenon seems to have been of greater apparent size, and therefore nearer to the observer. The separation by an obscure streak seems not to have been visible in England, perhaps from the change of its relative position.

The conclusion to which we come after all, regretting earnestly the want of French observations, is that we have here probably a meteoric object, moving, according to the calculations of Mr. H. D. Taylor (vol. xxvii. p. 140), with great velocity through the upper strata of the atmosphere and at the same time of auroral character, as the spectrum observation of Mr. Rand Capron (vol. xxvii. p. 84), makes out beyond any doubt. The separation and the feather-like forms, observed at Utrecht, make it probable that it was a mass of meteoric dust, passing through our atmosphere like an accumulation of little shooting stars. In this way the phenomenon of November 17 brings a confirmation of my own theory of auroræ, proposed by me in the "Appendice alle Memorie della Società degli Spettroscopisti Italiani," 1878, vol. ii., and received with sympathy

¹ Will Mr. W. M. Flinders Petrie be so kind as to tell us where we can find the Swedish observation mentioned by him, vol. xxvii. p. 140?